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Abstract

This paper analyzes the impact of a merger in the French retail sector on food prices, using a consumer panel data. We perform a difference-in-differences analysis by comparing price changes in stores for which the local market structure is affected by the merger to unaffected stores. In addition, we empirically investigate economic forces behind the observed price changes. On average, we find that the merger significantly raised competitors' prices contemporaneously with merging firms' price increases. Further, we show that competitor prices increase more in local markets that experience larger structural changes in concentration and chain differentiation.

Keywords: Ex-post merger evaluation, Retail grocery sector, Difference-in-differences.
JEL Classification: K21; L11; L66.

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1 Introduction

Over the last thirty years, successive merger waves have dramatically increased food retail sector concentration in most western economies. In 2000, in the US, the largest five retail groups realized close to one third of total food sales. According to the American Antitrust Institute, the number of supermarket mergers in the US has increased from 20 in 1996, to 25 in 1997, and to 35 in 1998 (Foer, 1999). In 1999 alone, the Federal Trade Commission (FTC) reviewed and approved two of the most important supermarket mergers: Albertson's acquisition of American Stores (the second and fourth largest chains in the US) and Kroger's acquisition of Fred Meyer. This second merger created the largest US grocery chain and the second largest retailer in the US in terms of revenue, behind Wal-Mart. Western European countries are also characterized by highly concentrated retail sectors that have become more concentrated, with merger waves happening since the 1980s. The highest concentration ratios are attained in the northern European countries, with the total market share for the largest three retailers (CR3) up to 90%.[1]

Supermarket mergers are a particularly important issue for antitrust authorities because food expenditures represent a large share of household budget - about 13% on average in European countries for 2012, and 7% in the US.[2] Large price variations due to a retail merger may cause a large impact on consumer surplus. When reviewing retail mergers, two particular features of the retail sector, namely the local dimension of competition and buyer power, make the antitrust analysis more complex. First, because supermarkets compete at the local level, the effects of a merger have to be analyzed for each local relevant market (see, e.g., Turolla, 2012). Second, antitrust authorities have to balance potential anticompetitive effects against efficiency gains due to synergies, as in all merger cases, but also against gains induced by buyer power. Indeed, the merged retailer is likely to obtain better terms and conditions from its suppliers, and to pass on part of this price reduction to consumers. Increased buyer power can thus lead to a welfare-enhancing reduction in final prices: this effect is specific to the vertical structure of the retail industry and explains why competition authorities may be more prone to clear mergers in the retail industry than in other sectors. For instance, between 1998 and 2007, the FTC approved 134 supermarket mergers for a total of 153 cases under investigation.[3] Among the 100 retail mergers proposed between 1990 and 2012 to the European Commission (EC), 89 were approved, 8 were approved subject to conditions, and only 2

[1] In 2004, the retail CR3 was 91.2% in Denmark, 79.6% in Finland, 81% in Iceland, 82% in Norway, and 91.2% in Sweden (Einarsson, 2007), while in 2003, the CR5 was 72.6% in France, 67.8% in Germany, 69.1% in Spain, 68.5% in Portugal and 63.5% in the UK. Note that in Italy, the retail sector remains rather traditional with a CR5 close to 40%.

[2] Sources: Eurostat (http://epp.eurostat.ec.europa.eu/statistics_explained/index.php/Household_consumption_expenditure_-_national_accounts) and USDA (http://www.ers.usda.gov/data-products/food-expenditures.aspx#.UpMmqhCPglA).

[3] See Table 4.2 http://www.ftc.gov/os/2008/12/081201hsrmergerdata.pdf.

were denied.[4]

The aim of this paper is to analyze retrospectively the impact of a merger among supermarkets on food prices in France. In 1999, the second largest retail group launched a take-over bid over the fifth largest retail group. This merger was approved by the EC and the French Competition Authority (French CA) in the year 2000. Together, the new group had almost 30% market share. The corporate decision to merge was made at the national level. The merging firms kept almost all their existing store locations, but rebranded two of the pre-existing retail chains. Our research question is twofold: First, we investigate whether this approved merger caused prices to increase. Second, we empirically assess potential economic forces inducing the price changes due to the merger.

We benefit from an exceptional database, which provides a unique setting to define local markets as catchment areas around each store, in order to capture the local dimension of retail competition. The data record food consumption and prices at the store level from a consumer panel (Kantar TNS-Worldpanel) and data on the French retail sector (location address and characteristics of the stores) for the years 1998-2001, i.e., before and after the merger. In our identification strategy and empirical analysis, we take advantage of the fact that, before the merger, the two merging firms were not operating in all local areas. Because the merger was approved at the national level, it was implemented in all local areas where merging firms were present. As a result, local markets were affected by the merger to the extent that the merging firms were in business there in the pre-merger period. In what follows, we refer to the merging firms as "the insiders" and to the other stores as "the outsiders". We define the control group as the set of outsiders' stores that do not compete directly or, indirectly, with a store belonging to the merging firms. The treatment group then comprises the insiders' stores, on the one hand, and the outsiders' stores located in the same catchment area as a store that belongs to insiders, on the other hand. In our estimation strategy, we quantify the price effects caused by the merger using a difference-in-differences approach. In particular, we compare price changes of outsiders in treated areas to price changes of outsiders in control areas. As we do not have a control group for the insiders, we examine their changes in prices that are correlated with the merger in a simple first difference approach. As the "pure" difference-in-differences may be affected if the treatment and control groups differ in the pre-period, we conduct an additional estimation approach using a propensity score matching estimator developed by Hirano, Imbens and Ridder (2003) and Imbens (2004).

Our results show that the approved merger affected competitors' prices positively and significantly, between 1.5% and 2.5%, and is correlated with insiders' prices increasing by 4 to 5%. By decomposing this effect even further, we show that while, on the one hand, the

[4]For instance, in 1997, the EC prohibited the merger between two leading food retail chains in Finland, Kesko and Tuko (see, 97/277/EC Kesko/Tuko (OJ L 110/53, 26/4/1997)). In 1999, the merger in Austria between Rewe and Meinl was allowed conditional on divestment of some stores (see, 1999/674/EC Rewe/Meinl (OJ L 274/1, 23/10/1999)).

merger is correlated with similar price increases for merging firms across all markets, on the other hand, competitor prices increase more in local markets that experience larger structural changes. These structural changes consist first in changes in the number of local competitors, resulting in higher concentration. Second, irrespective of changes in the number of competitors, the total number of chain names may drop in a local market, due to the rebranding operation, resulting in higher store differentiation.

This paper fits into a growing economic literature which attempts to evaluate whether approved mergers actually increased prices, in a context of some experts stating that the US antitrust policy towards horizontal mergers has been too lenient (Ashenfelter, Hosken and Weinberg, 2013). Historically, empirical mergers analysis goes in two main directions and there is a lively debate between the two approaches (Angrist and Pischke, 2010; Nevo and Whinston, 2010). First, some papers, in the spirit of Nevo (2000), build structural models of demand and supply in order to simulate mergers using pre-merger data. In the supermarket industry, Smith (2004) simulates retail structural changes in the UK and finds that retail divestitures reduce prices while mergers increase prices. A second stream of empirical papers uses both pre- and post-merger data on prices to directly estimate the effects of structural changes and mergers (such as Focarelli and Panetta, 2003 for retail banking; Hastings, 2004, Hastings and Gilbert, 2005, Taylor and Hosken, 2007 all three papers in retail gasoline; Hausman and Liebtag, 2007 and Basker and Noel, 2009 for retail entry; Duso et al., 2013 for book retailing; Ashenfelter and Hosken, 2010 for food and non-food grocery sectors; and Ashenfelter, Hosken and Weinberg, 2013 for the home appliance sector). Recently, Houde (2012) conducts both a retrospective analysis and a structural econometric simulation of a vertical merger in the Canadian gasoline sector, and reconciles both approaches.[5] Considering the US supermarket industry, Davis (2010) examines post-merger price changes using store-level scanner data and shows that chains reduce promotions after a merger, both in terms of depth and frequency. The most closely related study to date is by Hosken, Olson and Smith (2012), who examine the price effects of a large set of national US retail chain mergers occurring over a period of time. They find geographically heterogeneous price effects. The implication of these findings is that mergers should be analyzed at the local level, as we do. Our paper extends this stream of retail literature by taking advantage of an exceptional database at the store level, which enables us to causally identify localized price effects of a merger. The second contribution of our paper is to not only estimate the causal effect of a merger on prices, like previous related papers, but to test several economic mechanisms at play behind the price responses to a retail merger.

The rest of the paper proceeds as follows. Section 2 provides the background of the French retail sector, while Section 3 describes the data used. The empirical strategy is outlined in Section 4. In Section 5 we present and discuss the results. We perform several

[5] See also Weinberg and Hosken (forthcoming), Weinberg (2011), or Björnerstedt and Verboven (2012).

robustness checks in Section 6. Finally, Section 7 concludes and discusses some of the policy implications of our results and possible extensions.

2 Background on the French Retail Sector

We start by providing some background on the French food retail market structure and the regulatory environment, in Section 2.1. Next, Section 2.2 presents evidence on retail chains pricing strategies in the pre-merger period. We finish with an overview of the main facts about the merger, in Section 2.3.

2.1 Market Structure and Regulatory Framework

In 2000, i.e., before the merger, the French retail sector was already rather concentrated: the total market share of the five main retail chains (CR5) was close to 73%, a rather high concentration compared to the UK or Germany (respectively 64 and 57%). According to the French CA estimates, in the overall retail market, the joint market share of the two merging groups, henceforth called the insiders and denoted $M1$ and $M2$, was around 29.4%, while most of the remaining share was split between the largest rivals, henceforth called outsiders and denoted Oi, with $O1$ (15.4%), $O2$ (15.1%), $O3$ (13%), and $O4$ (9.9%).[6]

According to the standard categorization of stores, there are four main store formats in the French food retail sector. *Hypermarkets* are large grocery stores with a selling surface over 2,500 m^2, which sell both food and non-food products (on average, food accounts for at least one third of their sales). They are generally located outside of the main cities. *Supermarkets* are smaller, but located closer to the city centers: their selling surfaces range from 400 to 2,500 m^2. Compared to hypermarkets, these stores offer a reduced assortment of products, and are more specialized in food products (more than two thirds of their sales). *Convenience stores* have a selling surface below 400 m^2. Finally, *hard discount stores* are (usually small) supermarkets that carry a limited assortment of products, mostly sold at low prices and under their own brands.[7] In 2001, the food expenditure of French households was split as follows: 34.7% in hypermarkets, 29.9% in supermarkets, 8.5% at convenience stores, and 16.3% at specialized shopkeepers, such as butchers, and bakers.[8]

[6]Due to a confidentiality agreement with TNS Worldpanel, which provided us the data, we are not allowed to disclose the retailers' names. The French CA uses Nielsen data to compute these estimates. The report also displays the joint market shares by format provided by the two groups: 31.2% of hypermarket sales, 22.3% of supermarket sales, 16.1% of discounters' and 26.9% for the grocery retailing sector. Computing the market shares in terms of selling surface does not strongly modify these figures: in 1998, $M1$ owns 20.2% and $M2$ 10.3% of total hypermarkets surface, while for supermarkets these figures are 9.8% for $M1$ and 16.4% for $M2$, for discounters $M1$ has 15.1% and $M2$ 16.4%.

[7]In 2000, the market share of own brands in France was around 22.1% in volume and 19.1% in value (source: PLMA / Nielsen/ Allain and Chambolle, 2003).

[8]Source: INSEE, Tableaux de l'Economie Francaise 2002/2003.

Two laws, the Galland law and the Raffarin law enacted in 1996 have had a deep effect on competition and prices, and expert reports, as well as academic papers, point out that these two laws contributed to the reduction of retail competition. First, the Galland law aimed at preventing below-cost pricing. A side effect of this law was to allow for the use of price-floors in the retail sector, which encouraged a raise in retail prices (see Allain and Chambolle, 2011 for a study of the price-floor mechanism involved by the law).[9] Second, the Raffarin law increased administrative control of the opening of new supermarkets and of the extension of existing supermarkets. Experts also claim that the Raffarin law had a strong effect on retail competition. By increasing barriers to entry, this law has limited "organic" growth of retail groups, triggering important merger operations that have led to an increase in the retailers' market power. Besides, in 2002 the monetary change (French Franc disappeared as the Euro was launched on January 1, 2002) is also likely to have had an effect on retail prices.[10] In order to avoid these two sets of shocks that are orthogonal to the merger, we focus our merger analysis on the period 1998-2001.

2.2 Retail Pricing: Local versus National Pricing Strategies

An important characteristic of the retail sector is that, irrespective of global concentration ratios, on average local competitive conditions are related to final prices. In its first report on the sector, the French CA argued that: "The concentration of the retail food industry has little effect on the downstream market because competition is fierce among retail chains" (Competition Authority 1997, p.28). However, the position of the French CA has changed over time, and in more recent reports the authority expressed the view that retailers benefit from weak local competitive conditions and exert significant market power in local markets (see Bertrand and Kramarz, 2002; Competition Authority, 2007; Turolla, 2012).[11] In particular, it has been well documented by consumers' associations that retailers distort their offers locally, mainly by adopting local pricing policies. Biscourp, Boutin and Vergé (2013) corroborate this view in finding that price decisions in the French retail sector are partly made at the national level and partly at the store level. This contrasts with the main retail chains pricing strategy that sets uniform pricing at the national level in the UK.[12]

[9] For expert reports, see, e.g., Commission Hagelsteen (2008) or Allain, Chambolle and Vergé (2008) for a review.

[10] The introduction of the Euro has led to extensive discussion about its possible effect on inflation, and the economic literature points out ambiguous conclusions. Dziuda and Mastrobuoni (2009), for instance, show that, although the Euro changeover did not significantly increase inflation, it nevertheless had a distortionary effect on prices inside the Euro-zone. After the changeover, cheaper goods had higher inflation, and this effect was significant in France.

[11] A 2012 report by the French CA even calls for the right to impose ex-post remedies on retail groups when they are too highly concentrated in some areas, such as Paris (see Competition Authority, 2012).

[12] In 2004, the main retail chains in the UK, Tesco, Asda, Sainsbury's, and Morrisons, made a public commitment to uniform national pricing in the newspapers. For instance, Asda stated that "Asda pricing does not discriminate by geography, store size or level of affluence - we have one Asda price across the

Table 1: Regression of Prices on Local Markets Concentration

Dependent variable: (log) of mean price (by semester)					
Variable	(1)	(2)	(3)	(4)	(5)
Store size (m^2/1000)	0.0003***	-0.0001	-0.0002**	-0.0002**	-0.0002**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
log(market income)		0.0516***	0.0432***	0.0431***	0.0433***
		(0.0012)	(0.0015)	(0.0015)	(0.0015)
log(market population)			0.0014***	0.0013***	0.0013***
			(0.0001)	(0.0001)	(0.0001)
HHI (/10000)	-0.0100***	0.0079***	0.0160***	0.0161***	
	(0.0016)	(0.0017)	(0.0019)	(0.0018)	
HHI $\times$ $M1$					0.0012
					(0.0049)
HHI $\times$ $M2$					0.0400***
					(0.0051)
HHI $\times$ Outsider					0.0156***
					(0.0020)
Constant	7.5642***	7.0732***	7.1349***	7.1416***	7.1401***
	(0.0046)	(0.0127)	(0.0144)	(0.0143)	(0.0143)
Chain store FE	Yes	Yes	Yes	Yes	Yes
Semester FE	Yes	Yes	Yes	No	No
Product FE	Yes	Yes	Yes	No	No
Product-semester FE	No	No	No	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr	store-pr
R^2	0.960	0.960	0.960	0.961	0.961
Observations	1687782	1687782	1687782	1687782	1687782

Notes: Prices are collected over January 1998 and June 2000 (i.e., pre-merger period) and are expressed in centimes of French Francs (one centime equals 1/100 franc) per measurement unit (i.e., liter, Kg or unit). Promotional prices are excluded from the computation of average prices. All 1093 homogenous products are included in the sample. The *market income* variable corresponds to the mean household income calculated over the set of cities that belong to the catchment area of a given store. The *market population* variable is computed as the sum of inhabitants living in cities that belong to the catchment area of a given store. Clustered standard errors (at store-product level) are reported. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

Before turning to the merger details, we now present stylized facts on the pricing strategies for both the insiders and outsiders in the pre-merger period. In line with recent studies that have analyzed the correlation between local concentration and prices (see, e.g., Asplund and Friberg, 2002; Barros, Brito and de Lucena, 2006; Biscourp, Boutin and Vergé, 2013), we relate prices to variables controlling for the level of concentration in local markets. The purpose is to assess to what extent prices are set with regard to the level of local competition encountered. To do that, we exploit the TNS Worldpanel database (Kantar Worldpanel, 1998-2001) and collect prices for 1093 products (defined almost equivalently at the UPC level) sold in 7901 stores over January 1998 to June 2000. For each store, we define its catchment area by drawing a circle of 20 km radius around hypermarkets and one of 10 km radius around each of the other formats. Concentration in local markets is measured by the Herfindahl-Hirschman Index (HHI) computed based on selling surfaces at the retail group level. Note that each retail group is composed of

entire country". Dobson and Waterson (2005) provide a theoretical framework explaining why, under certain local market conditions, national retail chains are better off setting uniform prices.

several retail chains, each owning several stores. Controlling for unobserved components at the product and retail chain levels, we relate prices to local market conditions (e.g., income, population, or concentration level). The facts are presented in Table 1.

From Column (1) to (3), we gradually introduce distinct factors of local conditions: concentration (HHI), log of market income, and log of market population, while controlling for store size as well as semester, retail chain, and product fixed effects. In line with the aforementioned studies, the point estimate of the HHI variable testifies to a large average impact of local concentration on prices. In Column (4), we control for unobserved product-semester specific factors that can affect prices without this changing the correlation effects. Finally, we investigate heterogeneity in retailer's pricing strategies by interacting the HHI with insiders (decomposed between $M1$ and $M2$) and outsiders in a last specification presented in Column (5). We find that insiders do not have a pure local pricing strategy while outsiders do; in fact, $M1$'s prices on average are not significantly correlated with local concentration whereas $M2$ and outsiders' prices are on average significantly correlated with local concentration.

Based on these pre-merger period facts, two main insights will guide us in the merger analysis: (i) neither insiders nor outsiders have a pure national or pure local pricing strategy; in fact, pure within chain price dispersion measures are not zero for any of the retail chains; and (ii) $M1$ responds less than outsiders to local competitive factors.

2.3 The Merger

At the end of August 1999, $M1$ proposed a friendly take-over bid of $M2$. According to *Panorama Tradedimensions*, $M1$ was the second largest group in France based on store-surface market shares, while $M2$ was the fifth. After the merger, the new entity became the first group at the national level. The two groups were spread across 26 countries, but we focus on the French market, where they gathered around 220 hypermarkets and 1100 supermarkets. Henceforth, we denote by $M1_H$ and $M2_H$ the hypermarket chains and by $M1_S$ and $M2_S$ the main supermarket chains respectively owned by the groups $M1$ and $M2$. According to press releases, only 21% of $M1_H$'s customers also had visited a $M2_H$ store between July 1998 and June 1999, while half of $M2_H$'s customers claim to be occasional $M1_H$'s customers. The EC approved the merger on January 25, 2000, on the condition that $M1$ realize some divestments. It then delegated the decision to the French and Spanish competition authorities in order to assess the impact of the merger on retail competition at the local level. The French CA concluded that competition was likely to be affected in 27 local areas. However, the remedies required were not all enforced by the French Ministry of Economics, and the merger finally received final administrative approval on May 3, 2000.

In facts, the merger had a significant impact on concentration measures in the market

Table 2: HHI Before and After the $M1 - M2$ Merger

Panel A: Regional or National levels

	Paris	East	North	West	Central-W.	Central-E.	South-E.	South-W.	France
2000Q1	1599	1171	1261	1510	1430	1325	1498	1551	1214
2001Q1	2168	1242	1693	1735	1769	1683	1846	1811	1534
ΔHHI	+569	+71	+432	+225	+339	+358	+348	+260	+320

Panel B: Local market level

	p_{25}	p_{50}	p_{75}	Mean (S.E.)	Min.	Max.
2000Q1	1939	2424	3310	2939 (16)	1389	10000
2001Q1	2332	2658	3497	3180 (15)	1430	10000
ΔHHI	+393	+234	+187	+241 (5)	–	–

Notes: The table reports the Herfindahl-Hirschman Index (HHI) calculated at the retail group level for one semester before and after the merger. In Panel A, regions are defined according to the TNS Worldpanel classification. In Panel B, local markets are delimited with the baseline definition (20/10 km) used throughout the paper. The 25^{th}, 50^{th} and 75^{th} percentiles of the distribution of the local HHIs are reported. The variation between 2000Q1 and 2001Q1 is denoted by ΔHHI. The mean of the local HHIs is computed and its standard errors are reported in parentheses. For this last case, ΔHHI is computed as the average of the HHI variation observed in each local market.

during the period 1998-2001. Panel A of Table 2 displays the evolution of the Herfindhal-Hirshman Index (HHI) before and after the merger, at both the regional and national levels.[13] According to the EC horizontal merger guidelines, a merger is likely to raise competition concerns if the post-merger HHI is above 2000, while the variation is above 150.[14] At the regional or national levels, concentration is low enough for the merger to be approved without conditions. However, the local dimension of the retail market calls for a local assessment of the merger. For each store, we can compute a local concentration index (HHI) using the definition of local markets adopted in the previous Section 2.2. Panel B of Table 2 presents the distribution of HHI across local markets. Local concentration often appears clearly higher than the threshold recommended by the EC, and this explains why the EC referred to the French CA for an assessment of the merger at the local market level.[15]

Another important feature of this merger is that a substantial rebranding process took place among insiders. Before the merger, $M1$ operated stores under eight brand-chains: the hypermarket brand-chain $M1_H$, a main supermarket brand-chain $M1_S$ and $M1'$, which gathers all the other supermarkets, convenience stores, and discounters brand-chains. $M2$ operated stores under seven brand-chains: the hypermarket brand-chain $M2_H$, a main supermarket brand-chain $M2_S$, and $M2'$, which gathers all the remaining

[13]We do not have sufficient data to build the index upon real market shares. However, it is widely admitted that store sales are highly correlated to their selling area. Therefore, we base the concentration index on store surface area rather than turnover or quantities sold: the HHI in one market area is then the sum of the squared share of total retail surface for each retail group.

[14]See "Guidelines on the assessment of horizontal mergers under the Council Regulation on the control of concentrations between undertakings", 2004, III, § 16.

[15]Note that, overall, concentration seems to have increased mostly in areas with the lowest initial concentration (the first quartile of the HHI distribution increased by 393), while the increase in the most concentrated areas is less pronounced (the third quartile increased by 187). These data gather the effects of all market changes and not only of the merger we focus on.

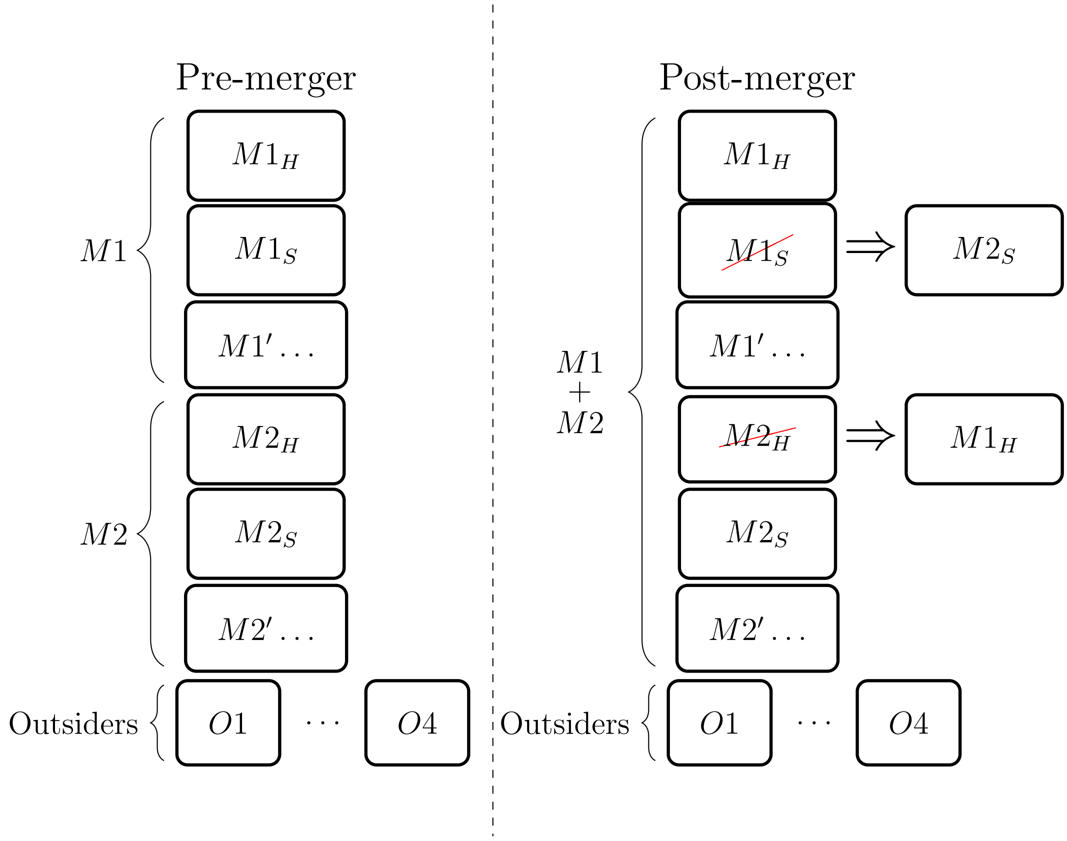
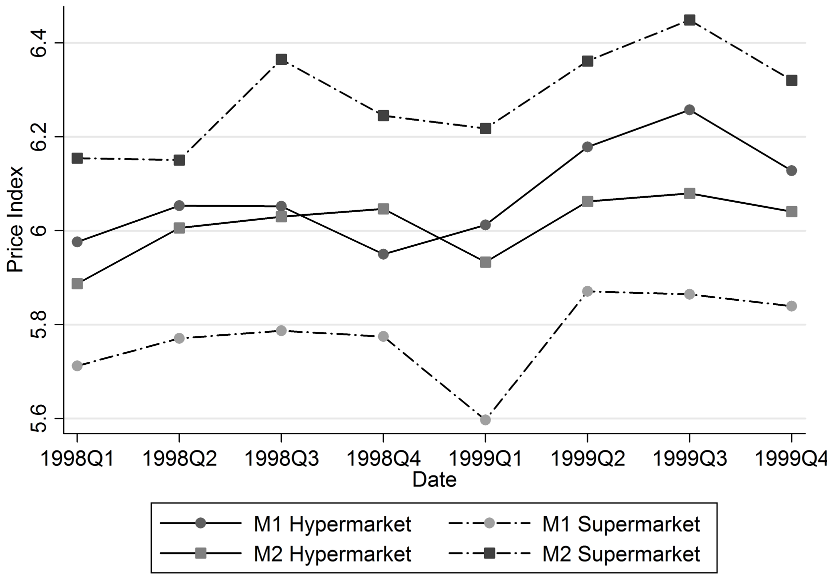


Figure 1: Rebranding Operations

supermarkets and convenience stores brand-chains.

As illustrated in Figure 1, hypermarkets $M2_H$ were rebranded into $M1_H$, while supermarkets $M1_S$ were rebranded into $M2_S$. Therefore, although $M1$ acquired $M2$, $M2_S$ supermarket chain remained active. This decision was motivated by a desire to keep hypermarket and supermarket chains with the highest brand image, as reported by press releases. In addition, Figure 2 reveals that the two chains $M1_H$ and $M2_S$ had a rather higher price-positioning than the other chains in the pre-merger period, suggesting that the rebranding operations had a significant impact on prices in the post-merger period.

Table 3 details the evolution of the rebranding operations. It shows that the merger was very progressively implemented by the two groups. The first rebranding of a $M2_H$ into $M1_H$ took place on May 31, 2000 and by August 2000, all the hypermarkets had been rebranded into "$M1_H$". The reorganization of the supermarkets took some more time (in August 2000, only half of the rebranding of supermarkets into $M2_S$ had taken place). The cost of rebranding a store is rather high, as it involves building work, changes in operation systems, and induced demand shocks. In 2000, $M1$ estimated the cost for rebranding a $M2_H$ into $M1_H$ as 75,000 to 150,000 Euros. The reorganization of the logistics system started at the end of 2000.



Notes: This figure plots the price trends of $M1$ and $M2$ hypermarket and supermarket chains during the pre-merger period. Each line corresponds to a price index of a retail chain for a given period of time. In order to cover a large share of food purchases, we relax the definition of homogeneous products used in the paper and select a basket of items based on a broader definition established at the product category level (e.g., yogurts, crackers, veal to roast, bananas), which enables us to additionally compute the price indices at the quarter level. We impose two selection criteria on product categories: a time-continuity of purchases over the pre-merger period and at least 10 observations per retail chain and per period of time. Overall, using this definition and the associated selection criteria, we deal with 138 product categories. The formulation of the price index is based on a weighted average of mean prices, where the mean prices of the product categories are calculated as an average revenue. Specifically, for a product category k, sold in retail chain c at period t, the mean price is computed as $\widehat{p}_{kct} = \sum_i p_{ikct}q_{ikct}/\sum_i q_{ikct}$, where p_{ikct} is the price of the $i-th$ observation of the product category k, sold in retail chain c at period t, and q_{ikct} is the quantity purchased. Then the price index for retail chain c at period t is computed as a weighted average $\widetilde{p}_{ct} = \sum_k \widehat{p}_{kct}\omega_k$, where the weight for each product category ω_k is calculated based on the share of the product category k in the total expenditure.

Figure 2: Price Indices by Retail Chain

3 The data

3.1 Data Sources

This study uses a unique dataset that combines information from three sources. The primary data are scanner data collected by the company *TNS Worldpanel* (Kantar Worldpanel, 1998-2001). This dataset records food purchases from a panel of households that are representative of the geographical and socio-economic group characteristics of the French population. The data contain detailed information on household characteristics, including the postal code of their home address, and all their purchasing activity during the year. Purchase data are collected by the households themselves by recording all their purchases with a home scanner. Information is reported at the level of the individual food product, and for most products these data are directly scanned from the barcode, making information available at the universal product code (UPC) level. We have information on prices paid and quantities purchased. Products are described by a rich set of

Table 3: A Time-Line Evolution of the $M1 - M2$ Merger

	1998				1999			
Number of stores	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
# of $M1_H$	116	116	132	132	132	132	132	132
# of $M1_S$	381	436	436	436	467	464	466	469
# of $M1'$	859	858	854	849	808	835	823	809
# of $M2_H$	77	78	83	84	84	84	85	85
# of $M2_S$	484	483	498	496	510	535	541	544
# of $M2'$	547	539	524	521	507	467	460	458
# of Outsiders	7104	7058	7045	7056	7070	7083	7090	7108
Total	9568	9568	9572	9574	9578	9600	9597	9605

	2000				2001			
Number of stores	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
# of $M1_H$	132	132	140	216	216	212	212	211
# of $M1_S$	471	475	351	144	1	0	0	0
# of $M1'$	797	797	794	798	797	799	789	790
# of $M2_H$	85	85	76	0	0	0	0	0
# of $M2_S$	547	543	669	877	1009	988	983	978
# of $M2'$	457	458	461	458	458	454	453	451
# of Outsiders	7123	7123	7122	7123	7139	7164	7177	7184
Total	9612	9613	9613	9616	9620	9617	9614	9614

Notes: The table presents the number of stores for each retail chain of the merging group and for all the outsiders, by quarter during the pre- and post-merger period (1998-2001). $M1_H$ ($M2_H$), $M1_S$ ($M2_S$), and $M1'$ ($M2'$) denote the hypermarket chain, the main supermarket chain, and all the other store chains of the merging group $M1$ ($M2$, respectively). Computed from *Panorama Tradedimensions*; authors' calculation.

characteristics. Overall, the data cover more than 400 categories of food products. In addition, households provide information about their shopping place, by filling in the store type (e.g., retail stores, convenience stores or specialized shops, and, inside retail stores, hypermarket, butcher, or delicatessen, for instance), the store size and, for retail chains, their name. For the purpose of this study, we consider the period that spans 1998 to 2001 - which corresponds to nearly to 32 million purchases.[16] We complement these data with information on retail store characteristics over the same time period, obtained from the *Panorama Tradedimensions* dataset. This dataset lists grocery retail stores that operate in France and gives information on their attributes such as store size (in square meters), format, chain name or store address, for instance. The dataset also reports information on changes in ownership, as well as opening, extension, or closing of stores. Lastly, we collect population and average household income information from census surveys, for the same time period, to proxy for determinants of demand faced by stores at the *commune* level (the French administrative unit similar to city).

Even though the *TNS Worldpanel* home-scan data provide one of the most detailed pictures of the French shopping habits for food products, the lack of information on the exact location of the store where the products are purchased prevents us from directly matching the purchase data with the dataset on store characteristics. We recover the missing information by combining data on the household address, the name of the chain

[16] A more detailed presentation of the home-scan data can be provided upon request.

and the size of the store where the purchase was made in the following way: we construct an algorithm which (1) defines the set of all candidate stores of the relevant chain around the household residence, and (2) selects the one that matches the store size reported by the household, or if several stores have the same size, selects the closest one among them. Having matched food purchases to the retail stores, we obtain a store-product level dataset covering around 27 million food product purchases.

We observe a large disparity in the frequency of purchases among products. For instance, bottled water represents 6.93% of the recorded purchases whereas chocolate bars amount to 1.81%. Within product categories, most of the UPCs correspond to a few observations. In fact, as for every home-scan panel data, we only observe a fraction of food sales in the population, making the tracking of products with low sales at the store level difficult. Consequently, we choose to aggregate the data at the semester level to account for a larger part of food products bought in France. Therefore, we compute for each UPC a mean price by semester expressed in centimes of French Franc (1 centime $\approx$ 0.0015 €). We follow recent studies using retail scanner data (e.g., Nevo, 2000), and calculate price as the ratio between French Franc sales and quantity purchased.

We restrict our attention to UPCs that satisfy some criteria of representativeness in order to compare prices over time and across stores affected or not by the merger. We also perform robustness checks on those criteria. First, we exclude infrequently sold products. For example, we exclude bretzel, which is only sold in the North-East of France. We also exclude products that are not present before and after the merger. For example, a new product launched after the merger is excluded from our sample. We exclude also products that are not present in both the affected areas and in areas not affected by the merger. According to this selection procedure, we identify 120 UPCs that gather both national brand products and fresh products (i.e., fruits, vegetables, meat and fish). To sum up, the dataset used in this study covers 120 UPCs sold in 619 stores over the period 1998 (pre) to 2001 (post). The information is aggregated at the semester level. The unit of observation in our analysis is an average price for a product, computed as a quantity weighted price over a semester in a certain market and retail store.

3.2 Local Market Definition

Assessing the price effect of the merger requires us to define the relevant market around each store. We base our definition of local competition on the *catchment area* of each store, i.e., the area from which most of the customers originate. Hence, the set of competitors for a store will be defined as the set of stores located inside this *catchment* area. The French CA assumed in this particular merger case that, on average, consumers are willing to drive from 15 to 30 minutes to reach a hypermarket, while they drive 10 to 15 minutes to a supermarket or to a discount store. In other retail merger cases, such as Rewe/Billa

Table 4: Market Structure of Local Markets

Fraction of catchment areas with *at least one*:	20/10 km Original dataset	20/10 km Final dataset
$M1_H$	46.41	50.89
$M2_H$	37.83	38.13
$M1_S$	43.27	47.33
$M2_S$	51.09	51.53
$M1_H$ & $M2_H$	25.90	24.72
$M2_S$ & $M1_S$	25.21	27.14
$M2_H$ & $M1_S$	23.14	23.91
Merging firms $M1 + M2$	84.28	87.40
Total number of catchment areas	9605	619

Notes: Local markets are delimited using the baseline definition of stores catchment area (20/10 km). The statistics on market structure are reported for the second semester of 1999 (pre-period). The percentages reported in the column labelled *Original dataset* are computed from Panorama Tradedimensions, which compiled global information for all of the grocery stores operating in France. The column *Final dataset* is based on a subset restricted to those stores for which the recorded purchases in the TNS Worldpanel satisfy some criteria of representativity over the period of study. The final dataset corresponds to the data used in the empirical analyses.

and Rewe/Meinl decisions, the EC states that: "These local markets can be defined as a circle with a radius of approximately 20 minutes by car centered on the individual sales outlet". Furthermore, it is generally agreed that hypermarkets have a larger catchment area than supermarkets.

In line with the position of the French CA, and converting driving time into kilometric distance, we define around each store a market area that spans up to 20 km for hypermarkets and up to 10 km for other formats, around the center of the city where a store is located. Thus, the set of local competitors for a given store consists of all the hypermarkets within 20 km around the city center where the store is located, and all other stores within 10 km. Since the distance traveled for a given driving-time varies according to the geographical features and urbanization, we test other definitions of local markets in the robustness section. Table 4 presents statistics on the configuration of local markets computed from the whole set of stores operating in France (labelled in the Table as "Original dataset") and also from the final dataset for which a recorded purchase satisfies the criteria defined in Section 3.1, used hereafter (labelled in the Table as "final dataset"). From the comparison of the original and final datasets, Table 4 shows that the final dataset quite closely reflects the structure of the French retail market. This table also shows that stores belonging to the merging firms are present in 87.4% of the local markets. The hypermarkets of the merging group are very well distributed over the national market, as half of the catchment areas contain a store $M1_H$ (50.89%), while around 38% have a $M2_H$; furthermore, they compete in only 25% of the areas. The supermarket chains belonging to the merging group are also present in around half of the areas, while they compete in around 27.14% of the areas.

4 Empirical Strategy

4.1 Methodology and Identification

Our goal is to estimate the price changes that result from the merger. A straightforward way to measure these price changes would consist of comparing the mean changes in prices, i.e., the average differences between pre- and post-merger prices, for stores impacted by the merger with the potential mean changes that those stores would have experienced if they were not affected by the merger. Since it is not possible to observe how prices would have changed "absent" the merger, we construct a counterfactual that reflects as closely as possible how stores would have reacted in the absence of the merger. We do this by taking advantage of the following quasi-experimental setting. Before the merger, $M1$ and $M2$ were not operating in all local markets; thus the merger at the national level did not have a direct impact on local competition in those markets. Thus, depending on whether the retailers located in a certain market, we are able to directly estimate the effect of the retail merger on food prices by comparing price changes in markets affected by the merger (treated markets) to price changes in markets unaffected by the merger (counterfactual control markets).

Building on the standard program evaluation literature, we postulate that there are two "states of nature" into which a product sold at a given store could have been assigned: the first state is such that a product is sold in a market where no store is affected by the merger and the second state is such that a product is sold in a market where the merger influenced the market structure. In the following, we estimate the effect of the merger on prices by comparing the changes in products' prices between the two states. To quantify the price change that results from the merger, we apply a difference-in-differences (DID hereafter) approach. The principle of a DID analysis is based upon the comparison of the average effect of a treatment (here the merger) on an outcome (here the prices), between two groups: the *treatment group* that includes subjects exposed to the treatment ($T = 1$) and the complementary group, called the *control group*, that includes subjects unexposed to the treatment ($T = 0$). Let $P_{ijt}(0)$ be the price charged by store i for a product j (at a non-treated store) at semester t and let $P_{ijt}(1)$ be the price under treatment, respectively. We are estimating the average treatment effect (ATE), which can be expressed as $\mathbb{E}[P_{jt_1}(1) - P_{jt_0}(1)|T = 1] - \mathbb{E}[P_{jt_1}(0) - P_{jt_0}(0)|T = 0]$, where t_0 and t_1 are the pre- and post-treatment periods, respectively. The simple estimate of the average treatment effect is performed by computing an unconditional difference-in-differences. The key identification assumption is that, absent the merger, the prices would have evolved identically between the two groups.

A natural definition of the treatment group is to consider stores affected by the merger either directly (i.e., stores belonging to the merging firms), or indirectly (i.e., outsiders located in the same local market as a store of the new entity). Hence, outsiders that

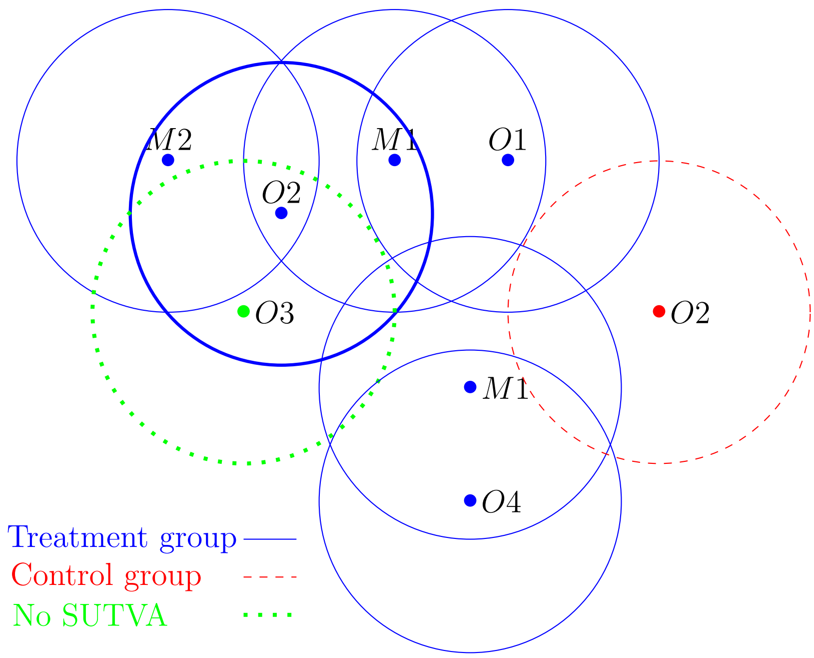


Figure 3: Definition of the Treatment and Control Groups

do not compete with a store belonging to the merging firms are included in the control group.[17] The treatment group is defined as all stores belonging to a local market where one insider is active during the pre-merger period.[18] Figure 3 illustrates the definition of treatment and control groups in a simplified local market with three stores belonging to the merging groups ($M1$ and $M2$) and five belonging to the outsiders. In Figure 3, all insiders, that is, $M1$ and $M2$, belong to the treatment group, as do neighboring outsiders (blue, solid circles), like $O1$, $O2$, and $O4$. The control group gathers all the stores whose catchment area is unaffected by the merger, that is, the outsider $O2$ (red, small dashed circle) in Figure 3. To satisfy the stable unit treatment value assumption (SUTVA), we exclude from the control group all stores whose catchment area includes an outsider that also belongs to a treatment catchment area, that is, a store like $O3$ in Figure 3.[19] Note that, if we use the baseline definition of local markets (d =20/10 km), more than 87% of the markets include one store of the merging groups (see Table 4). The treatment group will therefore be larger than the control group. In what follows, we will discuss several methods to correct this potential bias.

To ensure that the DID estimator identifies and consistently estimates the average effect, one may assume that assignment to treatment is independent of the outcome. Using the natural-experiment terminology, this means that assignment to a treatment

[17] The spatial dimension of retail competition makes it particularly difficult to draw the line between affected and unaffected markets. Several recent papers, such as Choné and Linnemer (2012), in the case of a merger in the Paris parking market, provide methods to improve the definition of the treatment and control groups.

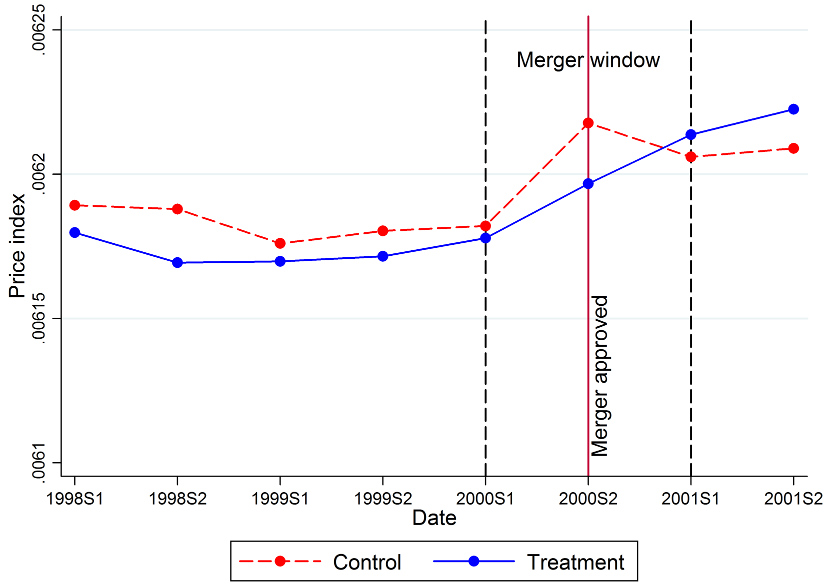
[18] The corresponding insiders are $M1_H$, $M2_H$, $M1_S$, $M2_S$ and $M1'$, $M2'$.

[19] This only happens for 9 stores, which are excluded from the control sample.

group is not confounded with the outcome (also known as the *unconfoundness assumption*, see Rosenbaum and Rubin, 1983). This estimate will be biased if factors that could affect prices vary significantly across treated and comparison markets. Unfortunately, the unconfoundness assumption is hard to sustain in the context of merger evaluation because treatment assignment is not random. This is particularly true for retail mergers, because firms decide where to locate stores according to markets characteristics. Given that the merger is decided nationally, the merger treatment is assigned based on the pre-determined location of the merging firms. Therefore, a concern is that locations of firms are endogenous and thus retailers that merge may be present in areas that are very different from the areas where the merging retailers are not located. For instance, firms that offered low quality items are more likely to settle in isolated low-income markets, while other firms may prefer to operate in more concentrated and wealthier markets. To account for this selection bias, it is usual to require unconfoundness "conditional" on a set of covariates that control for observed disparities between the two groups. According to this standard approach, we estimate the following regression using store-product level prices as the dependent variable:

$$\begin{aligned} \ln P_{ijt} &= \alpha_1 + \alpha_2 PostMerger_t + \alpha_3 T_i^d + \beta PostMerger_t \times T_i^d \\ &\quad + \delta' \mathbf{Z}_{it} + \mu_i + \sum_{n=1}^{N=j\times t} \lambda_n \tau_{jt} + \varepsilon_{ijt} \end{aligned} \tag{1}$$

where P_{ijt} denotes the average price charged by the i-*th* store, for product j during the semester t, $PostMerger_t$ is a dummy variable that identifies the post-merger period, and T_i^d is a dummy variable that characterizes store i as belonging to the treatment group, i.e., $T_i^d = 1$ when store i belongs to the insiders or competes, in a neighborhood d, with an insider. The regression also includes a set $\mathbf{X}_{ijt} = \{\mathbf{Z}_{it}, \mu_i, \tau_{jt}\}$ of observable covariates by store, product, and time. The idea is that store fixed effects μ and product-semester fixed effects τ control for, respectively, store factors that remain constant and affect price, and product-semester factors that vary and affect price. All these factors are uncorrelated, that is, exogenous, to the merger - the treatment. Further, $\mathbf{Z}_{it}$ are time-variant catchment area attributes of stores (e.g., local market income) that control for time varying market specific effects (e.g., local demand shocks). Despite the introduction of these market level factors, it is worth noting that unobserved shocks are still assumed to affect the outcome identically in both groups. Consequently, the average effect of the merger is captured through the coefficient vector β. We note that the vector β is an average of the price effects for merging and non-merging firms. Because it accounts for the merging firms' price effects, it cannot be interpreted as causal. This is because there is no control group, since insiders are absent from control markets. The "insiders" effect in β is interpreted as a correlation. However, if we just average the effect for the "outsiders", then it can



Notes: This figure provides a graphical illustration of the evolution of outsiders' prices in the treatment and control groups. For a given group, the price index is calculated as an average of the weighted mean prices of the UPCs, where the weights correspond to the share of the UPC in total expenditure.

Figure 4: Outsiders' Average (log) Prices by Treatment and Control Groups

be interpreted as the causal effect of the merger, as it is indeed a difference-in-differences point estimated effect. In order to clearly separate the type of price reaction that can be interpreted as a causal effect of the merger, we estimate the following regression:

$$\begin{aligned}\ln P_{ijt} \quad = \quad & \alpha_1 + \alpha_2 PostMerger_t + \alpha_3 T_i^d \\ & + \beta_1 PostMerger_t \times T_i^d \times O_i + \beta_2 PostMerger_t \times T_i^d \times (1 - O_i) \\ & + \delta' \mathbf{Z}_{it} + \mu_i + \sum_{n=1}^{N=j \times t} \lambda_n \tau_{jt} + \varepsilon_{ijt}\end{aligned} \tag{2}$$

where O_i takes the value of one if store i is an outsider.[20]

Because we observe only eighteen months of data after the merger approval, we concentrate on the short-term effect of the merger. This will enable us to distinguish competitive effects from long term structural changes outside the merger, which can affect prices in the long run, such as the monetary switch from the French Franc to the Euro in 2002, as well as unobserved efficiency gains from reorganization that can reasonably be expected to materialize in a few years. Similarly to previous retrospective merger analyses in retail markets (e.g., Focarelli and Panetta, 2003; Hastings, 2004 or Houde, 2012), we assume that there is no efficiency gain in the short term, but cost reductions due to renegotiation of supply contracts may be immediate. As we have seen in Table 3, the rebranding of stores took place gradually during the second half of 2000. This leads us to drop the data

[20] The above regression also includes the lower order terms of all the higher order interactions associated with the average treatments effects of interest, but we do not include them to save space in the equation.

for the second half of 2000 in order to avoid issues related to transitory shocks generated by the rebranding of stores. We also choose to remove data from the first semester of 2000 to leave data uncontaminated by a potential anticipation of the merger by the parties.

Another important issue when doing this DID empirical strategy is to make sure there are no differences in pre-existing trends for the treated and control areas. Figure 4 presents the time patterns of average (log) prices for outsiders belonging to the treatment and control groups, where prices are computed as a weighted average over products. Comparing the evolution of prices in the two groups, we first observe no significant difference in the price trends between the treatment and control groups in the pre-merger period, suggesting that the treatment and control stores share broadly similar price patterns in the pre-period. Looking into the post-merger period, it appears that the merger coincides with a larger price increase for the treatment group than for the control group.

4.2 An Alternative Estimator of the ATE

There are several potential identification issues with the reduced DID form specification presented above. First, if there is only limited overlap in the distributions of the confounding factors $\mathbf{X}$ across the treatment and control groups, and if the functional form assumptions are incorrect, missing outcomes will be incorrectly imputed. Estimates of average treatment effects can also be biased if control observations are not appropriately re-weighted to control for differences in the distribution of the set of variables $\mathbf{X}$ over regions common to the control and treatment groups.

To investigate this potential bias, we present, in Table 5, summary statistics on market structure according to the baseline definition of local markets. The table is organized into four Panels, A through D. In all four panels, Column (1) corresponds to the entire treatment group, Column (2) to insiders in the treatment group, Column (3) to outsiders in the treatment group, and Column (4) to the control group. Panel A reports the mean of population size, household income, and concentration measures in treated and control areas. Panel B reports summary statistics in terms of store characteristics, while Panel C reports summary statistics for the number of products and the number of purchases recorded. Panel D reports, for the pre- and post-period treatment, the average of the mean prices of the selected products sold in the treated and control stores. In the last four rows, Panel D reports the pure difference in average prices, labelled *Difference*, then the average difference-in-differences in the average prices for the treated and control stores labelled DD, followed by the average difference-in-differences for the outsiders labelled $DD_{Outsiders}$. Recall that it is not possible to compute a difference-in-differences for the insiders, as there are, by definition, no insiders in the control group.

The top three panels indicate that some factors, such as the average population, average HHI, and store characteristics, are different between the treatment and control groups.

Table 5: Summary Statistics on Treatment and Control Groups

	Treatment group			Control group
	All stores	Insiders	Outsiders	
A. Local market characteristics				
Average Population (in 1999)	633553	952271	492467	49397
Yearly average income per household	13959	14356	13783	12816
Average HHI	2496	2494	2497	4219
B. Stores characteristics				
Number of stores observed	541	166	375	78
Average store size (in m^2)	5461	7226	4680	2757
C. Products				
Number of homogeneous products	120	120	120	120
Number of purchases recorded	309592	106569	203023	35700
D. Average Prices & Average Difference in Mean Prices				
Pre-merger period (1998–1999)	3076	3187	3028	3074
	(275)	(282)	(273)	(278)
Post-merger period (2001)	3205	3257	3193	3156
	(279)	(284)	(278)	(279)
Difference	128	70	165	82
	(28)	(32)	(32)	(42)
DD			**46** (38)	
$DD_{Outsiders}$			**83** (39)	

Notes: Panel A, B, C, and D report market, store and purchase records summary statistics for treated and control catchment areas. The first column shows the averages of the variables in each row for all stores in the treated areas; the second column shows the averages for the merging retailers (insiders) in treated areas; and the third column shows the averages for the competitors (outsiders) in treated areas. Panel D reports, for the pre- and post-period treatment, the weighted average of the mean prices of the selected products sold in the treated and control stores, measured in centimes of French Francs. The last four rows of Panel D report the pure difference in average prices *Difference*. The row labelled DD corresponds to the average difference-in-differences for the treated and control stores over the selected products and $DD_{Outsiders}$ corresponds to the average difference-in-differences for the outsiders and control stores. Standard errors are reported in parentheses.

This comes in particular from urban areas, such as Paris, for which the baseline definition of local markets is rather large due to a high density of stores.[21] Turning now to Panel D, we find that the pure differences in the weighted average of prices are positive. Overall, we find that the average price change between treated and control stores is around 46 centimes. Looking just at the outsiders, we see their prices increasing more in treated areas than in control areas, by about 83 centimes of French Francs. This pattern also appears clearly in Figure 4. We warn, though, that, while these differences in averages are suggestive, we are not controlling for any events that could be happening in one market, but not another at the same time. To deal with this in the empirical strategy, we will perform a pure difference-in-differences panel data estimation strategy as expressed in Equation (2).

[21]Excluding the Paris area ("Ile de France"), the ratio of the average population of the treatment group over that of the control group is 4 instead of 13. Note that inequalities in average income, which were already weak, decrease further. By contrast the difference in HHI between the two groups remains high. To account for the heterogeneity in store density across cities, we investigate alternative definitions of local markets in the robustness section.

As we discussed, the pure difference-in-differences may be affected if the treatment and control groups differ in the pre-period. Given the differences observed in the top three panels of Table 5, we perform alternative comparisons for the markets affected by the merger through a semi-parametric matching estimator. More specifically, we use a propensity score matching estimator. As a first step, we estimate a probit of a merger occurring in a certain market where we include, as explanatory variables, store characteristics (such as store size), baseline factors that affect price trends (such as baseline concentration and competitors operating in the market), baseline factors that affect demand (such as the average income in the local area), and regional dummies. We then estimate the probability of being treated (of a merger occurring) as a function of these variables. In the second step, to control for differences in observed confounding factors between treated and control stores, we apply a re-weighting scheme proposed by Hirano, Imbens and Ridder (2003) and Imbens (2004). The basic idea is to use the fitted values of the probability of treatment from the probit analysis (the propensity scores) to re-weight the regression sample, effectively creating a smooth version of a match on propensity score. Let the propensity score S be the probability that a market in the data is impacted by the merger as a function of baseline characteristics. We re-weight observations in the non-affected sample by S/(1-S). This balances the distribution of baseline characteristics across the treated and non-treated markets. Intuitively, this technique up-weights data from markets that were not treated, but had a high probability of having been treated (having a merger occur) based on baseline observable data.

5 Results

Before entering into a detailed analysis we present a simple before-and-after comparison of prices in Section 5.1, where we control for market income effects as well as for store and product fixed effects. Next, in Section 5.2, we perform the DID analysis that enables us to estimate the causal effect of the merger on outsiders' prices. Finally, in Section 5.3, we lay out the potential sources of price changes for both the insiders and outsiders and then empirically investigate which of these sources can explain the observed price effects.

5.1 A Before-and-After Analysis

We present a before-and-after comparison of prices, similar to the row "Difference" of Panel D in Table 5, but now we control for market income, product, and store fixed effects. The results of the OLS regressions are presented in Table 6 where the first three columns differ based on whether we introduce the fixed effects and on their type. Column (4) replicates the estimation of Column (3) using observations weighted by the expenditure shares of food products at the national level. We find that prices have increased after the

Table 6: Before and After (OLS Estimates)

Dependent variable: (log) price (by product, by store, by semester)				
	No expend. weights			Expend. weights
Variable	(1)	(2)	(3)	(4)
PostMerger × Insider	-0.0963***	0.0419***	0.0431***	0.0520***
	(0.0305)	(0.0050)	(0.0050)	(0.0061)
PostMerger × Outsider	0.1172***	0.0619***	0.0631***	0.0787***
	(0.0146)	(0.0040)	(0.0040)	(0.0052)
log(market income)	0.0439	-0.0188	-0.0374	-0.1202**
	(0.1154)	(0.0487)	(0.0479)	(0.0599)
Constant	6.7810***	7.3797***	9.5035***	10.2902***
	(1.1017)	(0.4651)	(0.4569)	(0.5719)
Store FE	–	Yes	Yes	Yes
Product FE	–	–	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr
R^2	0.002	0.172	0.987	0.987
Observations	27900	27900	27900	27900

Notes: Stores catchment areas are delimited using the baseline definition (20/10 km). Data for the year 2000 are removed (i.e., event windows). Product-semester fixed effects are dropped to allow identification of point estimates of post-merger interaction terms. The weights used in Column (4) correspond to product expenditure shares computed at the national level during the pre-merger period. Clustered standard errors (at store-product level) are reported. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

merger both for the insiders and for the outsiders: looking at Table 6 (Column 4), we see that prices have increased significantly by 5.20% on average at the insiders' stores, while they have significantly increased by 7.87% on average at the outsiders. As we cannot perform a DID analysis for insiders, since there are no insiders in the control group, we know that the merger is correlated with an average price increase for the insiders.

5.2 Causal Effect of the Merger on Outsiders' Prices

We now present the results of the causal average effect of the merger on outsiders' prices in Table 7, where the dependent variable for all specifications reported in Columns (1)–(6) is the log of price (centimes of Franc) of product j sold at a store i during semester t. The basic structure of Table 7 is to present different estimation strategies in different columns. In Column (1), we report the estimates of the average treatment effect on the treated by computing an unconditional difference-in-differences. As this estimate will be biased if factors that could affect prices vary significantly across treated and control markets, Columns (2) to (6) report point estimates from different strategies. First, in Columns (2) to (4), we estimate a regression specification of the observed log prices on the treatment variables and also include a set $\mathbf{X}$ of observable covariates by product, store, and time. The idea is that product fixed effects, store fixed effects, and product-semester fixed effects control for, respectively, product specific constant factors affecting price, store constant factors affecting price, and product-semester varying determinants of price. All these factors are exogenous to the merger, that is, uncorrelated with the treatment. In Column

Table 7: DID and DID-Matching Estimates

Dependent variable: (log) price (by product, by store, by semester)						
	No expend. weights				Expend. weights	
Variable	(1) OLS	(2) OLS	(3) OLS	(4) Pure DID	(5) Pure DID	(6) DID-Matching
Merger × Outsider	0.0185***	0.0176***	0.0175***	0.0146***	0.0253***	0.0267**
	(0.0060)	(0.0061)	(0.0061)	(0.0053)	(0.0083)	(0.0126)
log(market income)	0.1153	-0.0099	-0.0285	-0.0426	-0.1018	-0.2332*
	(0.1172)	(0.0487)	(0.0479)	(0.0588)	(0.0750)	(0.1258)
Constant	6.2354***	7.2942***	9.4187***	9.5877***	10.1504***	11.3984***
	(1.1133)	(0.4653)	(0.4570)	(0.5608)	(0.7146)	(1.1977)
Store FE	–	Yes	Yes	Yes	Yes	Yes
Product FE	–	–	Yes	–	–	–
Product-semester FE	–	–	–	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr
R^2	0.006	0.173	0.987	0.988	0.989	0.989
Observations	27900	27900	27900	27900	27900	27900

Notes: Stores catchment areas are delimited using the baseline definition (20/10 km). The treatment group corresponds to catchment areas where $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during the pre-merger period (1998 and 1999). The control group corresponds to catchment areas where none of the previous retail chains operate during the pre-merger period. Data for the year 2000 are removed (i.e., event windows). The row Merger × Outsider corresponds to the interaction term PostMerger × Treatment × Outsider. The lower order terms of higher order interactions are not reported due to space limitations but are included in all specifications. Clustered standard errors (at store-product level) are reported. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

(5) of this table, we repeat the specification in Column (4) but weight each price by the share of each product in total expenditures in all stores, where the weights are computed using the pre-merger original dataset only. Finally, in Column (6), we turn to the semi-parametric estimator of propensity score matching. The parameter of interest is the one associated with the variable in the row labelled "Merger x Outsider", which isolates the observations made in stores that did not belong to the merging groups. Standard errors are clustered at the store-product level.[22]

According to the pure difference-in-differences, not controlling for anything else in Column (1), we estimate that the merger has a significant effect on prices of about 1.8% on average for outsider firms in affected markets relative to firms in unaffected markets. However, as discussed above, this merger estimate could be biased. Further, because the R^2 is low, we explain very little of the variation in prices with this specification in Column (1). When we control in Column (2) for store fixed effects, those explain 17% of the variation in observed prices; product fixed effects explain an additional 81% of the variation in prices, corresponding to the change in R^2 from Column (2) to Column (3). We therefore conclude that most of the variation in prices in the data is cross-sectional variation (98%). Among the remaining 2% of the variation, semester fixed effects explain very little of the variation in prices, as shown by the barely changing R^2 from Column (3) to (4).

[22] It is worth noting that the number of store-product pairs (4659 clusters) is large enough to correct any potential serial correlation issues in the computation of the DID estimates (see Bertrand, Duflo and Mullainathan, 2004, for a discussion).

The specification that is the most ambitious at controlling for any covariates that could affect prices is presented in Column (4). This is the covariate specification that we use henceforth in all additional tables (labelled "Pure DID"). Here we control for store fixed effects, as well as for product-semester specific varying factors, that can affect prices. In doing so, we estimate that the merger caused outsiders' prices to significantly increase by 1.5%. In this specification, we control for factors that could have changed semester by semester for each product separately. When compared to Specification (3), although the R^2 is very similar, the coefficient of interest drops significantly. The factors that could have changed could be, for example, changes in advertising at the national level for a given product that coincided with the post-merger periods or changes that would be common to all products within a category at a given semester, for example, if the number of manufacturers for a given product category drops in a post-merger semester at the national level (e.g., milk producers). That implies that those changes would be captured in the regression in Specification (3) by the merger treatment indicator. In Column (5), when weighting products by how much they get typically purchased, we again find a significant and positive average effect on outsiders' prices of about 2.5%. When compared to the smaller effect of Column (4), it appears that products with a high turnover (i.e., with high expenditure share) had the highest price increases due to the merger. When using a non parametric strategy in Column (6) with the label "DID-Matching", we find that the merger caused prices to increase by 2.7%, which is consistent with the results in Columns (4) and (5).[23]

While the results in Table 7 suggest that the merger caused outsiders' prices to increase on average, in the next section we empirically investigate the economic forces behind the observed price increases for both insiders and outsiders.

5.3 Investigating Different Sources of Price Variations

There are three main potential sources behind price increases due to this particular merger: concentration effects, differentiation effects, and pure rebranding effects. In Section 5.3.1 we define these possible mechanisms in the context of this merger event. In Section 5.3.2, we take advantage of the heterogeneous impact of the merger on different markets to examine whether there is evidence consistent with one or more of these mechanisms explaining price changes for insiders and outsiders.

[23]The propensity score probit estimates are available upon request. We also estimate the price effect of the merger using a nearest neighbor matching estimator. However, due to the common support assumption, we lose more than half of the treated stores, which reduces considerably the sample size and leads to non-significant point estimates. These results are available upon request.

5.3.1 Why Could a Merger Lead to Price Increases?

Concentration Effects A merger affects competition by suppressing a competitor, thus possibly affecting all firms' market shares. After this merger, concentration increased at both the national and local level. For instance, Figure 3 illustrates the case where a market, represented by a dark blue circle, is affected by this merger. In this example, the number of competitors in the dark blue circle changes after the merger, and we henceforth denote this as a Local Concentration Effect (LCE): prior to the merger, stores $M1$ and $M2$ were distinct competitors for O_2; after the merger, for O_2, the new entity $M1 + M2$ is a single group that owns two stores. In theory, the effect of the merger is as follows. The merging firms $M1$ and $M2$ internalize the competition effect and therefore increase their prices. In reaction, their competitors also increase their prices. Note that, while the example focused on a local market, as depicted in Figure 3, this effect may also be present at the national level.

Differentiation Effects Recall that, with the merger, two of the chains have changed their names: $M2_H$ was rebranded into $M1_H$ and $M1_S$ into $M2_S$. Therefore, at the national level, two chain names have disappeared. In local markets, that is not always the case since it depends on the geographical distribution of the stores in the pre-merger period. In the post-merger period we can have one of three situations: a drop in two chain names, labelled as "$\Delta N = -2$"; a drop in only one chain name, labelled as "$\Delta N = -1$"; or, finally, no drop at all, labelled "$\Delta N = 0$" even though there is rebranding. The first two cases are clearly illustrated in Figure 5. The third case is illustrated in Cases 2 and 4 of Figure 6. In this situation, it is possible that the loss of a chain name is offset by the fact that the new chain name did not exist before in this market, so the net change in names is zero.

The reduction in the variety of stores available to consumers may simply result in an increase in the horizontal differentiation among remaining stores. In theory, this differentiation effect is well illustrated in a Salop (1979) competition framework, where retail chains are located around a circle and consumers are uniformly located along the circle and incur transportation costs related to their distance to reach a store. In this model, the distance between stores represents the differentiation among chains. When two neighboring retailers merge, a drop in the number of chains could be modeled as a relocation of two previous stores into the same unique location. By relocating symmetrically around the circle, all firms would then obtain a higher market share because their two nearest neighbors are more distant. In equilibrium, the merger would then result in a price increase for all stores (e.g., Levy and Reitzes, 1992). Note also that, when rebranding stores, local demand of the merging firms may be negatively affected. By adopting the chain name of a previous competitor, a risk exists of disrupting the established connection between consumers and stores of the removed chain. For instance, inconveniences due to revamping stores (e.g., store layout) or the replacement of private labels by another brand may induce consumers to visit rival stores. Again, in the Salop circle framework, the total distance between the two nearest neighbors of a relocated merged firm decreases, which translates well into this loss in "potential" consumers. The differentiation effect, like the concentration effect, may impact firms either nationally or locally.

Pure Rebranding Effect Rebranding may have consequences in itself for both insiders and outsiders. Taking advantage of local market occurrences, as illustrated in Case 2 of Figure 6, as there is neither LCE nor a drop in N, we can interpret resulting changes in prices as due to the pure rebranding effect. Pure "rebranding" may still disrupt consumers habits by changing the chain name, as well as by any slight modification that can follow in a store's organization. It may thus affect outsiders who face a rebranded store in their catchment area. These outsiders may indeed gain new customers disappointed by the changes, or lose some customers wishing to change. We thus wish to isolate a "pure rebranding effect" corresponding to Case (2) in Figure 6. Note that, in the same spirit, Case (3) illustrates a pure local concentration effect without rebranding.

5.3.2 Empirical Evidence on Sources of Price Increases

In what follows, we investigate factors correlated with insiders' price increases and then sources causing outsiders' price increases.

Insiders After regressing the log of prices on product and store determinants, we project those residual variations in prices on an indicator for concentration changes interacted with the merger event.[24] We find that the merger is correlated with similar price increases of about 5% for insiders in areas where local concentration has changed and in areas where local concentration is unchanged after the merger. We also find no significant difference in price increases of insiders according to different levels of drops in chain names: that is for "$\Delta N = -2$", "$\Delta N = -1$", or "$\Delta N = 0$", which we called differentiation effects. Finally, pure rebranding is also not correlated with significantly different insider price changes. In sum, insiders price changes are not differentially affected by any of the potential sources we discussed. This result is consistent with the retail pricing strategy of $M1$ at the pre-merger period. As shown in Table 1, $M1$ was setting its prices independent of local competition conditions. When $M1$ acquired $M2$, $M1$ may thus have internalized the competition externality at the national level, which could explain why prices increased after the merger similarly at all merging stores. Moreover, due to the rebranding that took place after the merger, insiders have followed the pricing strategy of $M1_H$ and $M2_S$, which were the chains with the highest price positioning in the pre-merger period, as

[24] All results pertaining to the insider price changes are available upon request.

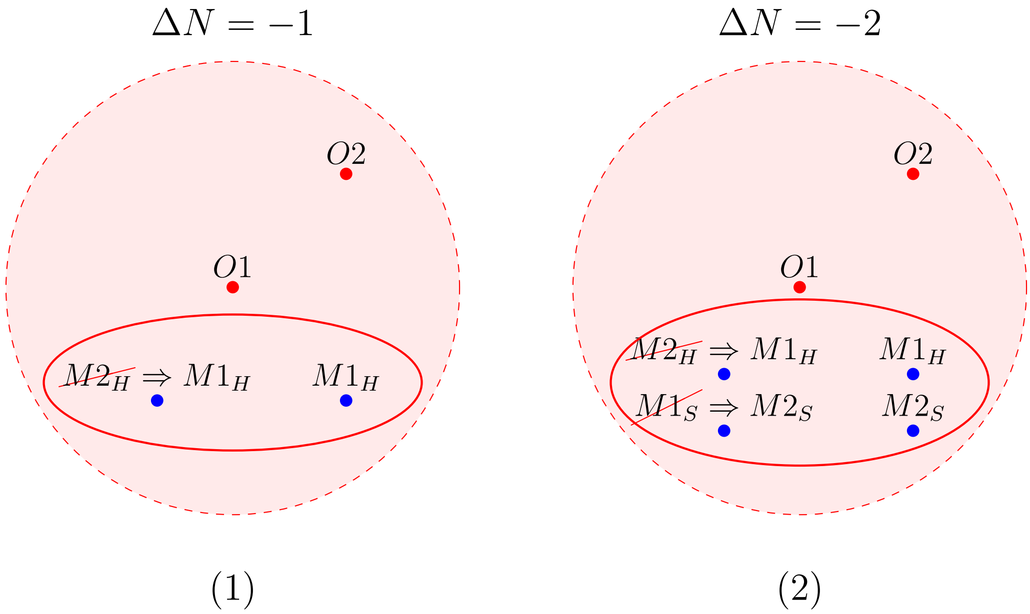


Figure 5: Drop in the Number of Retail Chains

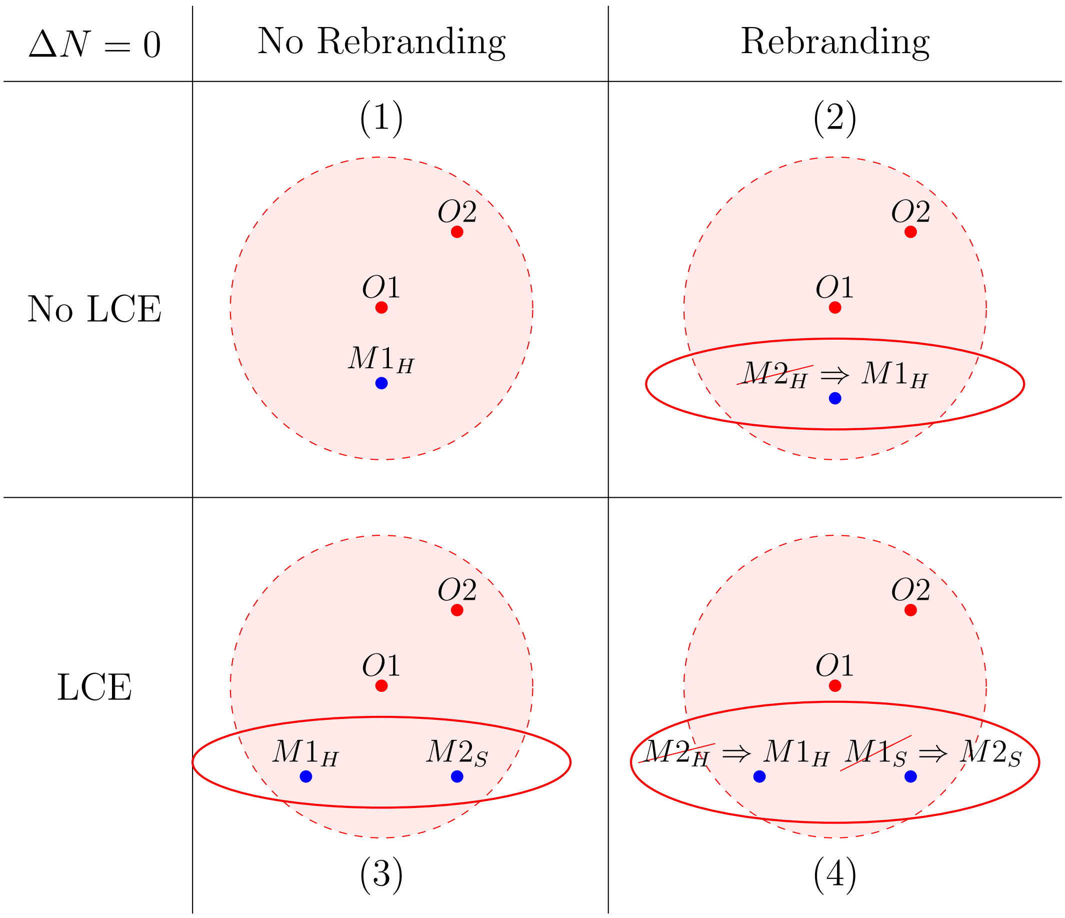


Figure 6: Local Concentration Effect and Rebranding Effect when $\Delta N = 0$

Table 8: Local Concentration Effect Estimates

Dependent variable: (log) price (by product, by store, by semester)				
	No expend. weights		Expend. weights	
Variable	Pure DID	DID-Matching	Pure DID	DID-Matching
Merger × Outsider × LCE	0.0217***	0.0206***	0.0328***	0.0336***
	(0.0058)	(0.0075)	(0.0086)	(0.0123)
Merger × Outsider × No LCE	0.0074	0.0059	0.0169*	0.0187
	(0.0059)	(0.0081)	(0.0091)	(0.0138)
log(market income)	-0.0402	-0.0954	-0.0985	-0.2307*
	(0.0589)	(0.0772)	(0.0749)	(0.1259)
Constant	9.7349***	10.2629***	10.1197***	11.3747***
	(0.5636)	(0.7395)	(0.7140)	(1.1993)
Store FE	Yes	Yes	Yes	Yes
Product-semester FE	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr
R^2	0.988	0.989	0.989	0.989
Observations	27900	27900	27900	27900

Notes: Stores catchment areas are delimited using the baseline definition (20/10 km). The treatment group corresponds to catchment areas where $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during the pre-merger period (1998 and 1999). The control group corresponds to catchment areas where none of the previous retail chains operate during the pre-merger period. Data for the year 2000 are removed (i.e., event windows). The row Merger × Outsider × LCE corresponds to the interaction terms PostMerger × Treatment × Outsider × LCE, where LCE is a dummy variable equal to one for market areas where there is a local concentration effect. The lower order effects of the merger are not reported. Clustered standard errors (at store-product level) are reported. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

shown in Figure 2. This may also explain the correlation between the observed insiders' price increases and the merger.

Outsiders Table 8 presents the empirical analysis for outsiders' price changes according to local concentration effects (LCE in the table). According to the pure difference-in-differences estimator (Column 1), we show that the merger caused a significant effect on outsiders' prices when the outsiders operate in markets where a LCE occurs. The estimated coefficient is statistically significant and reveals a price increase of about 2.2% in these markets. The results are unchanged in Column (2) when using the propensity score matching method. When re-weighting observations by expenditures shares, the effect of the merger on outsiders' prices in markets with LCE increases significantly by 3% in Column (3) and (4). In contrast, the merger does not cause a significant price increase in markets without LCE. This pattern is consistent with the hypothesis that outsiders change their pricing policy due to the increased local concentration.[25] This is also in line with our previous findings in Table 1, where we found that all the largest retailers, except $M1$, had pricing strategies correlated with local concentration levels.

In what follows, we attempt to isolate the differentiation effect that may result in a drop in the number of retail chains observed in several local markets. We denote by ΔN the *net* variation of the number of retail chains that an outsider faces. Note that, as

[25] If outsiders were not responding to local competition, then the DID point estimate would yield a zero treatment effect; this is because the change in control areas would be equal to the change in treated areas.

Table 9: Differentiation Effect Estimates

Dependent variable: (log) price (by product, by store, by semester)				
	No expend. weights		Expend. weights	
Variable	Pure DID	DID-Matching	Pure DID	DID-Matching
Merger × Outsider × $\Delta N = -2$	0.0279***	0.0258***	0.0392***	0.0393***
	(0.0075)	(0.0088)	(0.0102)	(0.0130)
Merger × Outsider × $\Delta N = -1$	0.0128*	0.0124	0.0236**	0.0246*
	(0.0070)	(0.0086)	(0.0103)	(0.0138)
Merger × Outsider × $\Delta N = 0$	0.0114**	0.0102	0.0218**	0.0236*
	(0.0057)	(0.0078)	(0.0087)	(0.0133)
log(market income)	-0.0433	-0.0973	-0.1003	-0.2321*
	(0.0587)	(0.0772)	(0.0748)	(0.1260)
Constant	9.7630***	10.2790***	10.1362***	11.3880***
	(0.5623)	(0.7394)	(0.7129)	(1.2000)
Store FE	Yes	Yes	Yes	Yes
Product-semester FE	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr
R^2	0.988	0.989	0.989	0.989
Observations	27900	27900	27900	27900

Notes: Stores catchment areas are delimited using the baseline definition (20/10 km). The treatment group corresponds to catchment areas where $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during the pre-merger period (1998 and 1999). The control group corresponds to catchment areas where none of the previous retail chains operate during the pre-merger period. Data for the year 2000 are removed (i.e., event windows). The rows Merger × Outsider × ΔN correspond to the interaction terms PostMerger × Treatment × Outsider × ΔN. $\Delta N = -2$ is a dummy variable equal to one for market areas where there is a net loss of 2 retail chains, and dummy variables $\Delta N = -1$ and $\Delta N = 0$ correspond to a net loss of 1 retail chain and no loss, respectively. The lower order effects of the merger are not reported. In the $\Delta N = -2$ case, 58 outsider stores lost one hypermarket chain and one supermarket chain simultaneously. In the $\Delta N = -1$ case, 28 outsider stores lost one hypermarket chain and 52 outsider stores lost one supermarket chain. Clustered standard errors (at store-product level) are reported. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

previously discussed, "$\Delta N = 0$" does not mean that none of the stores in the considered area have rebranded.

While from Table 7 (Column 4) we estimate in the pure difference-in-differences that outsiders' prices increased on average by about 1.5%, Column (1) of Table 9 shows that outsiders' prices increased more when their catchment area was affected post-merger by a larger drop in the number of competing retailers. When "$\Delta N = -2$", prices increased by 2.8%. When "$\Delta N = -1$", prices increased by 1.3%. The smallest price increase estimates of 1.1% appear for the case "$\Delta N = 0$". The results are similar when we use a semi-parametric estimation, and also similar if we repeat this investigation weighting each observation by the pre-period expenditure shares. The estimates reported in Table 9 suggest, though, that changes in the number of retail brand-chains competing with outsiders due to the merger are not the only force at play; indeed, we still find outsiders increasing their prices by 1.1% even when "$\Delta N = 0$".

Although treated outsiders who face a drop in the number of retail brand-chains in their catchment area are necessarily competing with a store that rebrands after the merger, the opposite is not true. A catchment area where rebranding occurs but where "$\Delta N = 0$" is illustrated in Cases (2) and (4) in Figure 6, as mentioned previously. The next table, Table 10, aims at decomposing this effect further. This table is a repeat of the previous

Table 10: Local Concentration Differentiation and Pure Rebranding Effect Estimates

Dependent variable: (log) price (by product, by store, by semester)				
	No expend. weights		Expend. weights	
Variable	Pure DID	DID-Matching	Pure DID	DID-Matching
Rebranding				
$\Delta N = -2$, LCE	0.0281***	0.0259***	0.0393***	0.0393***
	(0.0075)	(0.0088)	(0.0102)	(0.0130)
$\Delta N = -1$, LCE	0.0131*	0.0126	0.0237**	0.0247*
	(0.0070)	(0.0086)	(0.0103)	(0.0137)
$\Delta N = 0$, LCE	0.0169	0.0163	0.0281*	0.0300
	(0.0110)	(0.0120)	(0.0157)	(0.0184)
$\Delta N = 0$, No LCE	0.0094	0.0080	0.0205**	0.0215
	(0.0067)	(0.0085)	(0.0100)	(0.0139)
No Rebranding				
$\Delta N = 0$, No LCE	0.0051	0.0036	0.0130	0.0155
	(0.0072)	(0.0093)	(0.0108)	(0.0156)
$\Delta N = 0$, LCE	0.0290***	0.0282***	0.0409***	0.0420***
	(0.0091)	(0.0104)	(0.0125)	(0.0153)
log(market income)	-0.0458	-0.1001	-0.1002	-0.2329*
	(0.0588)	(0.0774)	(0.0747)	(0.1265)
Constant	9.6188***	10.1300***	10.1359***	11.3961***
	(0.5600)	(0.7369)	(0.7120)	(1.2048)
Store FE	Yes	Yes	Yes	Yes
Product-semester FE	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr
R^2	0.988	0.989	0.989	0.989
Observations	27900	27900	27900	27900

Notes: Stores catchment areas are delimited using the baseline definition (20/10 km). The treatment group corresponds to catchment areas where $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during the pre-merger period (1998 and 1999). The control group corresponds to catchment areas where none of the previous retail chains operate during the pre-merger period. Data for the year 2000 are removed (i.e., event windows). The first six rows (i.e., ΔN) correspond to a decomposition of the interaction term PostMerger × Treatment × Outsider. They are labelled as such to minimizing space. The lower order effects of the merger are not reported. Clustered standard errors (at store-product level) are reported. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

two, but now we break up the estimates by rebranding and no rebranding situations. The pure rebranding effect (corresponding to the row "Rebranding, $\Delta N = 0$, No LCE") does not cause a significant impact on outsiders' prices for any specifications except Column (3)'s specification of DID (when using weighted data), for which the pure DID is around 2.1%. These effects are robust for all other specifications, using the propensity score matching method in Column (2) and when re-weighting with the expenditure shares in Column (4). We thus conclude that a pure rebranding effect cannot explain the observed outsiders' prices increase.

6 Robustness Checks

Our results demonstrate that outsiders have raised their prices following a change in their market structure generated by LCE and differentiation, while insiders prices are not correlated with any local market changes induced by the merger. We now assess the robustness of our findings with respect to two central hypotheses used in the baseline specification: the definition of stores' catchment area following the 20/10 km radius circles and the absence of an anticipation of the merger beyond one semester. Finally, we investigate whether we find evidence for vertical buyer power effects of the retail merger.

Robustness to Catchment Area Definition Table 11 repeats the main specification for three additional definitions of a catchment area, resulting in four panels. In the first panel (labelled Specification 1), we consider a relatively large size for catchment areas, and delimit local markets around city centers where stores are located using a 30 km (20 km) radius for hypermarkets (supermarkets), respectively. The second panel (labelled Specification 2) corresponds to the baseline definition (20/10 km) and the results are reported for ease of comparisons. In Specification 3, we reduce the distance boundaries compared with the baseline definition and we adopt a 10/5 km definition, which may be more appropriate for densely populated areas where traffic congestion significantly reduces the distances traveled. Finally, we consider in a last specification a mix of the two previous definitions by using the 20/10 km definition overall, except for stores located in the most populated areas, where we adopt the 10/5 km definition.[26] In Columns (1) and (2), we do not weight observations by the expenditure weights, in contrast with Columns (3) to (4). Columns (1) and (3) report the DID estimates controlling for store fixed effects as well as for product-semester specific fixed effects, while Columns (2) and (4) present the estimates using the DID-matching approach.

Overall the results are robust to alternative market definitions. Varying downwards the definition of local markets (Specification 3) does not change the sign and the statistical significance of the merger effect on prices. Interestingly, when using a narrower definition of local markets (Specification 3), fewer stores are affected by the merger, which mechanically increases the size of the control group. As a result, the comparison between the treatment and control groups occurs between stores with more comparable characteristics. Applying the DID-matching approach (Columns 2 and 4), we obtain substantially higher point estimates, suggesting that stores that are now unaffected by the merger have moderately increased their prices. It is also worth noting that the estimates derived under Specification 4 are similar to those obtained with the baseline definition, which may be interpreted in two ways: either the 10/5 km definition is still too large for highly densely populated areas, or the merging firms are less present in these local markets compared to other areas. By contrast, the use of the extended definition of local markets (Specification 1) does not lead to statistically significant results, which may be explained partly

[26]The most populated areas are defined at the “département” (French administrative unit) level and correspond to stores located in one of the following “départements”: Bouches-du-Rhône (13), Rhône (69), Paris (75), Seine-et-Marne (77), Yvelines (78), Essonne (91), Hauts-de-Seine (92), Seine-Saint-Denis (93), Val-de-Marne (94), and Val-d’Oise (95).

Table 11: Alternative Definitions of Catchment Areas

Dependent variable: (log) price (by product, by store, by semester)				
	No expend. weights		Expend. weights	
Variable	Pure DID	DID-Matching	Pure DID	DID-Matching
Specification 1: 30/15 km (587 treated stores, 29 control stores)				
Merger × Outsider	0.0024	-0.0173	0.0038	-0.0209
	(0.0075)	(0.0106)	(0.0107)	(0.0140)
Specification 2: 20/10 km (Baseline, 541 treated stores, 78 control stores)				
Merger × Outsider	0.0146***	0.0135*	0.0253***	0.0267**
	(0.0053)	(0.0074)	(0.0083)	(0.0126)
Specification 3: 10/5 km (464 treated stores, 159 control stores)				
Merger × Outsider	0.0174***	0.0241***	0.0248***	0.0351***
	(0.0045)	(0.0071)	(0.0064)	(0.0116)
Specification 4: 20/10/5 km (537 treated stores, 82 control stores)				
Merger × Outsider	0.0146***	0.0132*	0.0257***	0.0257**
	(0.0052)	(0.0070)	(0.0080)	(0.0118)

Notes: Specification 1 corresponds to catchment areas delimited with the 30/15 km boundaries. Specification 2 corresponds to the baseline scenario where catchment areas are delimited with the 20/10 km boundaries. Specification 3 corresponds to catchment areas delimited with the 10/5 km boundaries. Specification 4 corresponds to catchment areas delimited with the 20/10/5 km boundaries. The treatment and control groups are defined according to the baseline definition. Data for the year 2000 are removed (i.e., event windows). The rows Merger × Outsider correspond to the interaction term Post-Merger × Treatment × Outsider. The lower order effects of the merger are not reported. All the regressions include store and product-semester fixed effects. Clustered standard errors (at store-product level) are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

by the low number of stores not affected by the merger. Taken together, these results stress the importance of choosing a relevant definition of local markets when conducting retrospective merger analyses in retail markets.

Robustness to Anticipation Concerns The baseline specification has been estimated removing one semester before and after the merger, in order to prevent a short term anticipation effect due to the merger. In our case, the merger was announced in the press nearly one year before the approval, suggesting that the parties could have coordinated their actions well before May 2000. In an attempt to evaluate whether our results are sensitive to a longer anticipation, we consider an alternative econometric specification that compares the level of prices on the long-difference between 1998 and 2001. Basically, the purpose of the long-difference specification is to confront the long-run equilibrium outcomes before and after the merger, which eliminates all possible biases yielded by an anticipation of the merger, and more generally by any transitory shocks occurring during the period. By contrast, the baseline analysis conducted with the full panel may suffer from understated estimates if the merging groups anticipate the operation and raise their prices before the event window.

We regress the difference in (log)prices between the last period of the panel (2001S2)

Table 12: Long-Difference Estimates

Dependent variable: $\Delta_{01S2-98S1}$ (log) price (by product, by store)				
	No expend. weights		Expend. weights	
Variable	Pure DID	DID-Matching	Pure DID	DID-Matching
Treatment × Outsider	0.0180**	0.0096	0.0285**	0.0139
	(0.0076)	(0.0087)	(0.0119)	(0.0140)
Treatment × Insider	-0.0042	-0.0125	0.0052	-0.0082
	(0.0082)	(0.0090)	(0.0122)	(0.0137)
Δ log(market income)	-0.0739	-0.0761	-0.1197	-0.2080
	(0.0825)	(0.0938)	(0.1020)	(0.1341)
Constant	0.0551***	0.0607***	0.0485***	0.0692***
	(0.0098)	(0.0114)	(0.0141)	(0.0183)
Product FE	Yes	Yes	Yes	Yes
Clustered errors	store	store	store	store
R^2	0.218	0.265	0.151	0.169
Observations	4650	4650	4650	4650

Notes: Stores catchment areas are delimited using the baseline definition (20/10 km). The treatment and control groups are defined according to the baseline definition. Clustered standard errors (at store level) are reported. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

and the first one (1998S1) for each product j sold in store i:

$$\Delta P_{ij} = \alpha + \beta_1 T_i \times O_i + \beta_2 T_i \times (1 - O_i) + \delta' \mathbf{\Delta Z}_i + \gamma_j + \varepsilon_{ij} \tag{3}$$

where $\Delta P_{ij} = lnP_{ij}^{01S2} - lnP_{ij}^{98S1}$, T_i is the dummy variable equal to one for stores belonging to the treatment group, and β_1 is the coefficient measuring the price merger effect for outsiders. The long-difference regression also controls for the change of market characteristics $\mathbf{\Delta Z}_i$ during the period and accounts for product-specific fixed effects γ_j.

Table 12 presents the estimates for the long-difference specification. Using the pure-DID methodology (Columns 1 and 3), we obtain quite similar results compared to the full panel specification, even though we control at a different level for unobserved product heterogeneity and heteroskedasticity in standard errors. The estimated merger effect shows that prices have increased significantly for outsiders stores affected by the merger: on average about 1.8% and even higher (around 3%) when using data weighted by product expenditure shares. These results reinforce our previous findings and demonstrate their robustness regarding potential temporary confounding factors and/or an anticipation of the merger. When using non-parametric matching, the point estimates are of similar economic magnitude, but are no longer significant, maybe due to lack of power.

Waterbed Effect Although we cannot directly investigate the effect of the merger on insiders' buyer power (as we do not have a control group), we can still investigate whether we find evidence consistent with a waterbed effect. Larger size retailers may obtain greater discounts from their suppliers, who in turn impose higher wholesale prices on smaller retailers. This so-called "waterbed effect" has been the subject of investigations by competition authorities (e.g., Competition Commission, 2008) and both empirical and

Table 13: Waterbed Effect Estimates

Dependent variable: (log) price (by product, by store, by semester)		
	No expend. weights	Expend. weights
Variable	(1)	(2)
Post-Merger	0.0707***	0.0907***
	(0.0148)	(0.0241)
Post-Merger × HHI	-0.0271	-0.0340
	(0.0273)	(0.0456)
HHI	-0.0625	-0.0754
	(0.0672)	(0.0898)
log(market income)	-0.2157*	-0.4225**
	(0.1128)	(0.1712)
Constant	11.1973***	13.1271***
	(1.0859)	(1.6494)
Store FE	Yes	Yes
Product FE	Yes	Yes
Clustered errors	store-pr	store-pr
R^2	0.988	0.988
Observations	2940	2940

Notes: The sample only includes outsider stores belonging to the control group. Clustered standard errors (at store-product level) are reported. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

theoretical papers (e.g., Genakos and Valletti, 2011; Inderst and Valletti, 2011). We test for potential waterbed effects by taking advantage of local concentration as a proxy for local competition. Assuming that, following the merger, suppliers have increased their wholesale prices towards outsiders, we expect that outsiders would pass some of this wholesale price increase through to consumers. They would pass through differently across markets that vary in local competition, that is, the increase in retail prices would be larger in more concentrated areas. To isolate such a waterbed effect from other effects (such as the reaction of outsiders to a change in insiders' prices after the merger) we focus only on the outsiders in the control group. Indeed, as buying strategies are more national than local, a waterbed effect would also arise in the control group. By running a before and after analysis on these stores only, we see in Table 13 that local concentration does not significantly explain outsiders' price increases, and therefore we reject a waterbed effect as a possible explanation of the outsiders' prices increase.

7 Conclusion

In this paper, we take advantage of a national merger between two French retailers, which impacted local market structure differentially depending on the pre-existing set of retail competition, to estimate the causal effect of this retail merger on retail prices of competing retailers. We find that the merger causes an average 1.5 to 2.5% price increase at competing retailers (the outsiders). We break up the overall increase in the outsiders' prices and find that a change in local concentration and a drop in local retail differentiation explain a large part of the treated outsiders' price increase. In contrast, isolating a pure rebranding effect, which appears in markets where one of the merging firm rebrands after the merger, but where no store of the other merging group operates (to avoid any local concentration effects), and where no store of this new brand was operating before the merger (to avoid a drop in local differentiation) does not explain significantly the treated outsiders' price increase. Even though we are unable to estimate the causal effect of the merger on insiders, due to lack of control groups, we find that the merger is correlated with price increases of the merging firms. Further, the price increases do not differ across local markets. Moreover, using the heterogeneity in the way the merger affects the treated markets, we find that insiders price changes do not respond to changes in local market structure. We infer from these results that retailers have a combination of national and local price setting strategies and that outsiders have more local pricing strategies than insiders.

The estimated price increase has important implications for consumer welfare. As food expenditures amount to approximately 12.9% in the European Union (on average, as of 1999), and as supermarket chains account for around 70% of total food sales in France (66.6% in 2010, INSEE), a back-of-the-envelope calculation shows that a 2.4% increase in supermarket food prices roughly represents a 0.2% drop in consumer purchasing power. Obviously such a simple calculation has to be taken with caution, as we do not take into account the effect on non-food prices and other services, but it gives an idea of the possible impact of such a merger on welfare.

In terms of competition policy, one of the major challenges is to be able to assess the impact of an approved merger on prices. Because the causal effect of the merger that we estimate is based on the outsider prices only, the overall price effects could be larger, given that the insiders' prices have also increased. A second challenge is to predict the potential price effects at the time when antitrust authorities are notified of a merger, in order to impose relevant remedies and to better protect consumers. In this setting, a retrospective merger analysis is not possible. Several approaches could be taken in this direction. First, using our detailed data, we can perform a simple prediction of how the local concentration changes induced by the merger would affect local market retail prices. Using the estimation results of Table 1 (Column 4), we perform an out-of-sample price prediction, given the post-merger local HHI levels. We find a predicted price increase of 2.11% with the new HHI, with a standard error of 0.05%. We conclude that these predictions using a simple method based on the variation in the local HHI index are rather close to the 2.5% price increase obtained in our expenditure weighted DID specification. Hence, using the HHI as a preliminary screen for merger analysis appears to be an attractive tool - a finding consistent with Hosken, Olson and Smith (2012). Second, but more time consuming than the first approach, we could compare the results of our retrospective analysis with those obtained following a more structural econometric approach as, in Houde (2012), which is an extension for future work.

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